

THE HOWNAM CULTURE: A REJOINDER TO RITCHIE

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It has been agreed that the criticism of my views expressed in the last paper should be answered briefly and I am grateful to Dr Anna Ritchie for letting me read the typescript of her lecture before its publication. A new theory must inevitably provoke discussion and sometimes dissent: a science without argument would be sterile. However we must ensure that any such disagreements are founded on the facts and on an accurate understanding of what the new hypothesis is suggesting. It is also, I submit, necessary that the refutation of a theory, if it cannot point to errors of fact, must be accompanied by an alternative and better explanation of the data. Only if these rules are observed will the ideal result emerge - the blending of the knowledge and ideas of the various interested workers to produce a generally acceptable synthesis.

We may consider first the question of factual errors and then the more general problem of the framing of theories of archaeological cultures. However the first point to make is that the article objected to¹ was a very brief outline summary of the new situation presented by a number of radiocarbon dates, all obtained since 1966, and of a new theory which I have evolved to explain them. It was an inappropriate target for a detailed attack. The full exposition of the part of the offending hypothesis dealing with the Abernethy culture is currently being prepared, but these comments will serve as an interim reply.

Turning now to the allegations of errors in, or avoidance of, facts the attack on the idea of a Hownam culture in the foregoing paper credits it with notions which were not expressed and thereby implies that its author has been guilty of careless thinking. The critic says (p.57 above), 'it (the culture) does not, apparently, include the stone-built forts or earthen forts ... with which it shares the same few artefact types. Are we really to believe that there was a complete break in cultural tradition between the first and successive phases of occupation at Hownam?'. This implies that the Hownam evidence was ignored to make the culture appear more plausible and homogeneous. Yet I actually said the very opposite (MacKie 1969, 15-16), namely, 'The possibility that the bronzes and even the different forms of forts themselves, were on the whole simply adopted by pre-existing communities was always real but has not had the attention it deserves, though the stratigraphy of sites like Hownam Rings in Roxburghshire suggested in 1948 that it might be true. There the primary wooden palisaded enclosure was replaced by a fort with a single drystone wall which was in turn replaced by a small multivallate hillfort, yet there was no

evidence of any drastic change in the culture of the site's occupants'. Also the chart of the suggested regional population groups on fig. 2 clearly shows that phases 2 and 3 at Hownam Rings fell into the Late phase of my Hownam culture.

Secondly Dr Ritchie says that the Hownam culture is by me 'defined by a single type of domestic site ...' and that 'to make a culture out of one structural type ... is as unacceptable as would be a culture defined by bronze axes'. The latter part of this statement is itself highly questionable. The remains of domestic sites and structures are far more reliable witnesses - even by themselves - of the customs and character of a resident prehistoric population than bronze axes, which could easily be the products of travelling professional smiths who habitually crossed tribal boundaries. However the immediate point is that I did not say that the palisaded sites alone defined the Hownam culture. The second paragraph of the section Palisaded Sites: the Hownam Culture (MacKie 1969, 21-2) draws attention to the great disparity between the material cultures of the palisaded (and other native) sites north of the Tyne and that from Staple Howe, the 'superficially similar' site to the south of that river, which yielded abundant and quite different pottery, exotic (Hallstatt) bronzes and probably evidence for corn storage implying an unusually efficient farming economy. Thus not one but three important diagnostic characteristics were given for the early Hownam culture even in that brief summary account - namely the palisaded settlements themselves, the poor material culture which contrasts with that of neighbouring regions and the distinct geographical zone in which this combination is found. That there is a clear northern as well as a southern frontier to the Hownam culture-area is implied by the inclusion of the Craigmillar Wood palisaded site in Renfrewshire with the Abernethy culture to the north because of its different material culture (MacKie 1969, 18-19, fig. 2). Thus it is perfectly obvious that not all palisaded sites fall into my Hownam culture and that therefore a culture cannot be defined by them alone from this basic fact. In any case, as I explained earlier, I made it clear that the Tyne/Forth palisaded sites were characteristic of the Hownam culture only in its Early phase: later other types of settlements were in use. The whole point of the article was to suggest cultural continuity over many centuries.

A third objection to the theory is that 'it is difficult to distinguish between the Hownam and Abernethy cultures as defined on other than geographical grounds' because several of the allegedly diagnostic artefacts (including palisaded sites) are common to both 'cultures'. If true this would indeed constitute a grave objection to making two separate contemporary cultures out of the Late Bronze and Early Iron Age archaeological material of the North-eastern and Tyne-Forth provinces, though it would still not necessarily be an objection to the

actual creation of an archaeological culture of some sort therefrom, the prime target of the criticism. (Incidentally, it is a strange argument that denies the existence of culturally diagnostic artefacts and then uses the nature of these artefacts to imply cultural similarity between the two zones.) The justification of the Abernethy and Hownam cultures as I define them needs far more space than is available here and must be held over². For the present it must suffice to say that, while there is admittedly some overlap of the diagnostic artefacts at the frontier between the two culture areas (broadly the two geographical provinces mentioned above), it still seems to me on the basis of available evidence that the archaeological material in these two zones is on the whole clearly distinct, though we know it to have been contemporary. The only exceptions are the hammerstones which crept in by accident (MacKie 1969, 19); they are so common on so many Iron Age sites that they are probably useless as cultural indicators.

When we also take into account the spread of intrusive pre-Roman ornaments and implements into these two areas - some Late Bronze Age bronzes, beehive rotary querns, glass beads, bronze rings, pins and fibulae as well as the more aristocratic Iron Age metalwork - I believe this reinforces the view that there were two distinct peoples north and south of the Forth/Tay zone. Some of this latter evidence is shortly to be available³, and although much of it is circumstantial - taken together it makes quite a strong case in favour of the theory of two cultures. And of course we know that in historical times the southern boundary of the Pictish peoples was more or less on the river Forth and there is less and less archaeological evidence as time goes on for doubting that the Picts were descended from their Iron Age forerunners⁴.

Probably the most important-sounding criticism is that, in terms of Childe's definition of an archaeological culture⁵, neither the Abernethy nor the Hownam culture as I define them exist because of the scarcity of recurring assemblages of artefacts. No one doubts that the criticism is literally valid as far as the Tyne-Forth province is concerned (though it is not, in my view, fairly applied to the Abernethy culture); the scarcity of finds from the palisaded and other sites is well known. But are we therefore to assume that we are helpless to proceed further in our theorising because the unfortunate prehistoric population with whom we are dealing failed to measure up to Professor Childe's 1929 criteria? I think not. What after all was Childe saying? He obviously did not mean that a prehistoric people cannot have existed unless all these concrete relics of its activities survived to come to our notice. He meant that we should not be reasonably certain that a population had a shared language and culture (in the anthropological sense) without the survival of plenty of this concrete evidence in the manner he described. A general point may be made first.

Archaeological evidence today is much more varied and diverse than it was forty years ago. Then prehistory was reconstructed almost entirely from structures and artefacts; now we have available a wide range of techniques for recovering the evidence from many sources - from tree and cereal pollen, the expert analysis of metals, stone, organic remains and so on, and from atomic science for dating. This kind of evidence for the activities of a prehistoric people is just as useful and relevant for the reconstruction of cultures as that derived from the surviving artefacts. In certain cases it could actually contribute more to our knowledge of a culture than the latter. Hence the concept that artefacts alone can define a culture is not valid today and still less can it be used to set limits to what can be inferred from archaeological evidence.

A more specific point is that the consistent absence of artefacts from excavated domestic sites can also be a useful diagnostic cultural characteristic, especially when - as in the case of the Tyne-Forth sites - this scarcity of material culture contrasts with the situation in adjacent regions. Similar inferences from negative evidence have been used to suggest a distinctive Iron Age population in Devon and Cornwall⁶. An archaeological culture is, one assumes, the remains of a socially homogeneous people and if direct evidence indicates the existence of a population in a finite territory over several centuries - as it does with the Hownam people - then this, when taken together with the small amount of culturally diagnostic evidence and all the circumstantial evidence, allows us to infer the existence of an archaeological culture, albeit a poor one. Admittedly this would be a culture with a lower level of plausibility than one defined in the classic Childean manner - through recurring assemblages of abundant artefacts - but it is surely time that it was recognised that hypotheses have different levels of plausibility according to the type of evidence on which they are based and that we should accept and define these levels rather than simply refuse to believe in, or even think about, the less clear material which does not fall at once into an obvious framework. Looked at in this way we can see that the counter arguments are essentially negative. None of the evidence for a homogeneous population inhabiting a specific area for a long period of time is denied - just the contrary. Yet it is argued in effect that no conclusions can be drawn because neither is the evidence in the approved category nor does it come up to the desired level of the clearly obvious. This is the crux of the argument: Dr Ritchie states firmly that there is no Hownam culture in the sense defined by Childe. I maintain that some prehistoric populations can be detected and defined to a certain extent in alternative ways and from circumstantial evidence even if abundant artefacts are not present and that, on the evidence available, a Hownam culture on a lower level of plausibility than Childe's definition can be said to be a reasonable hypothesis at the moment.

It must also be remembered that to deny the plausibility of an hypothesis is not in itself an impartial or neutral act but is a positive statement with implications of its own, one of which is that there is a better explanation of the facts (assuming these not to be in dispute). It is important to remember that a description of archaeological data is not a substitute for the writing of pre-history, only a preliminary to it. To deny the Hownam culture as I define it is not just to say that it does not exist in Childe's sense and therefore does not exist at all. It is to imply that there was no continuing, culturally homogeneous population with Bronze Age antecedents in the Tyne-Forth province. What was there then? One should not try to refute a theory without revealing how the data can be better explained. If it can be better explained I shall be the first to subscribe to the new view.

Notes

1. MacKie, E.W. (1969). 'Radiocarbon Dates and the Scottish Iron Age'. Antiquity, XLII (1969), 15-26.
2. It is hoped that a paper on the Abernethy culture will appear in P.S.A.S. See also note 5, especially Map 3.
3. MacKie, E.W. (1970a). 'English Migrants and Scottish Brochs'. Glasgow Arch. J., II (1970) forthcoming.
4. A summary of the situation appears in: MacKie, E.W. (1970b). 'The Scottish Iron Age': a revision article on the final prehistoric age in Scotland'. Scot. Hist. Review, XLIX (1970), 1-32.
5. Childe, V.G. (1929). The Danube in Prehistory, v-vi.
Childe, V.G. (1956). Piecing together the Past, 16.
6. Thomas, A.C. (1966). 'The character and origins of Roman Dumnonia', in Thomas, A.C. (Ed.), Rural Settlement in Roman Britain. C.B.A. Research Report No. 7, 74-98, especially fig. 4.